APPENDIX G

PROJECT MOHAVE FINAL REPORT INDEPENDENT PEER REVIEW COMMENTS

The reader should note in viewing the comments from the Peer Reviewers, that their comments were made from a review of the Project MOHAVE Draft Report. All of the comments and issues raised by the reviewers were considered and incorporated as appropriate into the Project MOHAVE Final Report.

Review of Project MOHAVE Final Report

By

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General Comments

The standards of review that I used were based on Mary Ann Allan's letter of October 1, 1998. They were the following:

1. Technical Soundness
   This I interpreted as the same standard used in reviewing scientific manuscripts. It is essentially the question in Mary Ann's letter of "are the assumptions valid, analytical methods sound and conclusions defensible?"

2. Report Presentation
   I started with the question of "are the conclusions accurately and understandably conveyed by the text?" I tried to apply the standard of a formal presentation to an EPA official.

First, a disclaimer. I am considered a rather tough reviewer. I hope that the technical committee understands that I am not trying to pick on them - but that I generally reject more scientific papers than I accept.

I found the report to be basically technically sound. The logic of the report is the following:

I . There was no correlation between the tracer from the Mohave Power Plant (MPP) and the extinction measured at Meadview. However, there was a correlation with relative humidity and the tracer from Los Angeles. (I understand the argument that this does not mean there is no effect from MPP. White et al 1998).
2. The TracerMax approach yielded a maximum impact during the intensive period of 23% (based on 10% conversion, some mass to scattering assumption and the measured extinction coefficient). I assume that there was no significant dispute of this number or it would have been reflected in the report.
3. The debate then is simply how much lower than 23% is MPP's e ffe c t.
4. Given the widely different models used, the 90th percentile range of 1-5% appears to be reasonable.

Basically, this is all the report needs to say. Most of the rest of the report appears to be filler demonstrating that a lot of work was done. As a result, much of the material should be moved to appendices.

I found the report presentation to be abysmal. This report reads as if sections were dumped on someone's desk and they made a valiant, but unsuccessful, effort to stitch them together. There is very little continuity, each sections reads as if a different person wrote it. While some difference in style is unavoidable in a report this long, an effort should have been made to have some consistency.

The authors seem to assume that only the Executive Summary and the Conclusion will be read. As a consequence they appear to have been written by committee. Needless to say, this was an excruciating and deathly boring report to read. I have lots of comments on this also, but since my primary job was not to be a technical editor, my comments are relatively brief.

As a general comment, it is not clear where the report represents consensus and where fuzzy language is used because various factions could not agree. What appears to have happened is that the various factions contributed their sentences and they were all included. This makes the report difficult to read. The EPA people that read the report will understand that the various groups have different points of view.

Someone needs to take charge and give the report more focus. If the various factions don't like it - let them include a dissenting opinion in an Appendix.

Major Technical Comments

My first major technical comment is that there is a problem with the measurement of the aerosol radiative properties. The single scattering albedo (the ratio of scattering to extinction) reported by the report on page 66 (~0.6) is more typical of a polluted urban area. I have attached a copy of a recent review by Heintzenberg et al (1997) showing a 0.7 value from Malm et al. (1996) in Figure 1. Heintzenberg et al (1997) conclude that the 0.7 value is an "unrealistic outlier."

The Report mentions that the absorption measurements could be off by a factor of 2. The specific issue of the IMPROVE absorption measurements was discussed by Hufftnan 1996a,b (and commented on by Horvath 1996). Huff-man claimed that the absorption measurements were not wrong - it was the elemental carbon measurements. Horvath disagreed (as do I). Needless to say, the issue has not been settled (Heintzenberg et al 1997) and won't be any time soon.

The MOHAVE Report need not get into this debate. The Report creates its own problem by using calculated extinction coefficients (humorously called a "reconstruction") as alternative way of presenting the results. The Report should just use the measured transmissometer values for extinction and include an error estimate

(perhaps mentioning that the calculated values underestimate the extinction coefficient). Then the Report doesn't have to get into the problems of the absorption coefficient measurement and the fact that the extinction coefficient calculations are less than the transmissometer values.

My second major technical comment is that all the "models" listed in Table A of the Executive Summary and discussed in the Report are treated as if they are equal. This is reflected in the presentation of the range of values. While this finesses the problem of some people objecting to their model being considered less appropriate it is also stupid. There should be some identification in the Executive Summary of which models gave which results. Again, it will not be a surprise to the EPA people that different groups have approaches that give different results.

My third major technical comment is actually my complements to the people who did the computer simulation of the scenes. I thought this was excellent. Unfortunately, it became almost irrelevant since the estimated impacts were so low and got very brief mention in the report. The only concern I have is that there needs to be some "test case" that can be used to evaluate if the computer display and presentation is accurate. But I thought the ability to switch between different scenes was wonderful. The description of the computer simulations in the Report seems out of date (scenes didn't seem to match with pg 159 etc; reference to photographs, etc). I assume that this part will be rewritten.

Specific Comments - page by page

Executive Summary

Pg i. First paragraph, line 4: The pronoun "It" appears to be the same as "the draft MOHAVE report" making the sentence unclear.

Pg i. Second paragraph, line 4: The term perfluorocarbon tracer is abbreviated to PFT in Table A of the summary - PFT should be defined here.

Pg i Fourth paragraph, carryover sentence: The term "highly uncertain results" is unnecessarily pejorative. The sentence could be reworded to say $_{64}$ model predictions are limited by the knowledge of the complex ... "

Pg ii First paragraph, last sentence: One problem I had with the Report was understanding why after no correlation was found with the MPP tracer and extinction coefficient at Meadview, the project didn't just end. Instead, the people waited several years and started a new round of analysis. So in 1998 (almost 1999) there is a report discussing 1992 data. This senter1ce did nothing to clear up the mystery. Since I don't know the answer I can't suggest a better sentence.

Pg iii First paragraph, first sentence: "new and refined" sounds like a commercial. Just use "the methods listed in Table A" or some other more neutral term.

Pg iii Second paragraph, second sentence: "conceptual model" is pompous. Just use "... a description of the conditions required.."

Pg iii Second bullet, first sentence: OK, one of my pet peeves is the expression "we know that." It is sprinkled throughout the report.

Pg iv Table A: The Table is somewhat confusing since it has so much information in it. Originally, I thought it should be moved - but I think it helps more than it hurts (it looks intimidating). I would try to trim it a bit and reference the more detailed discussion in the text.

Pg iv bullet: One thing that confused me initially was that there were two tracers released. Here and the next bullet should use MPP tracer.

Pg v second bullet: The term "no pattern of association" is a weasel term. if the "analysts" mean correlation - then say it. I assume there was a fair amount of debate on the subject. But this term is not the solution.

Pg v third bullet: If the analyses are not summarized below, then reference them, i.e. White, et al (1998)

Pg v fourth bullet: Again "we know that". This sentence would make my English teacher roll over in her grave.

Pg v fifth bullet: Again "we know that". This bullet has obviously been fought over and reads like it. Perhaps a reference to the Tracer Max calculation would help.

Pg v last bullet: I hope Ian Sykes will help reword this mess. I think the bullet is trying to say that given a highly complex, turbulent field, prediction of individual events is highly uncertain.

Pg vi first bullet, third sentence: This sentence is idiotic and embarrassing just delete it.

Pg vi third bullet Per my comments above, the contribution should not be presented in two ways -just use the measured values. Don't get into debates when you don't have to.

Pg viii Table E: This should be deleted.

Pg ix second full bullet, second to last bullet: This sentence is unnecessarily shrill. Just call it an upper bound. The EPA people will know what you mean. Sentences that use "our understanding of the physical atmosphere" are stupid.

Pg ix: last bullet (finally): The last two sentences are so vague as to be virtually meaningless. Is it so hard to state what statistical tests the analysts' used? I think the second to last sentence means correlation - but I'm not sure. The last sentence seems to imply that the high values predicted by "some of the models" (which ones?) do not appear in the data. Since this is the last bullet - It is important to get it right.

Table of Contents

Pg x subsection heads: Yes, I even have criticisms of the Table of Contents. Actually, it shows the lack of consistency. Some subheads are questions (What is Light Extinction?) and some are not. While perhaps useful in breaking the monotony, it is unnecessary and Section 9 (Source Contributions) comes off as condescending.

Chapter 1

Pg 1 first sentence: The MOHAVE project was/is longer than one year. Only the field program was one year long.

Pg 2 3 lines from bottom: "a" should be "at"

Pg 3 second paragraph goal 5: Should be "attempt to reconcile" the interpretations - there doesn't appear to be complete consensus. ,

Pg 3 last paragraph, 4th line from bottom: I may be splitting hairs here but presenting a range from different models is not consensus. Consensus would be if one model was given the most credibility. This report does not do that.

Chapter 2

Page 4 sixth full line from bottom: Should be Page, AZ

Pg 5 figure 2- 1: This figure needs work. MPP should have a white dot where the plant is. It would be helpful to put in the Colorado River.

Pg 6 first line: Should be southwestern

Pg 8 third paragraph: should be m s-1 not ms-1

Pg 10 second paragraph: The term is "calculated" not "reconstructed." The Sisler et al report is an internal report. If it needs to be referenced, it should be included as an Appendix. The same is true of the other internal reports (UC Davis, SCE, AER etc). The paragraph and Table 2-2 don't say what wavelength.

Chapter 3

In general - much of this chapter could be trimmed by moving a lot of the tables, etc into an appendix.

Pg 11 third line from bottom: space missing
Pg 12 TABLE 371 Move this to an appendix. Please!

Pg 18 and beyond. Universities do not make measurements - instruments make measurements and researchers set them up. "Harvard" did not analyze the data - a researcher did. It sounds stupid.

Pg 30 first line: What is "NGN" ?

Pg 30 first paragraph: Meadview has a transmissometer and is not mentioned.

Pg 31 Figure 3-9: same thing - the transmissometer at Meadview is not shown.

Chapter 4

Same general comment about moving stuff to the Appendix

Pg 37 section 4.1.3 Light Absorption: As a stated above, there is a serious problem with this measurement. The Report should reference reviewed works such as Heintzenberg et al (1997) and Horvath (1993).

Pg 44 first full paragraph: Horvath (1996) presented an alternate explanation for the absorption problem. If the Report references Huffman It should also reference Horvath's comments. The 10 M^2/g number is not as universally accepted as the Report indicates. But again, the Report should stay out of the debate.

Chapter 5

Again, much of this could be trimmed - use Appendices.

I discussed my problems with the absorption measurements above, so I will try to curtail my intense dislike of this chapter. I will only comment on the most objectionable parts.

Pg 66 throughout: It is not clear what wavelength the values are given for (I assume it is 550 nm). The absorption of "light" is not solely due to $N0_2$. There is H_20, 0_3, $C0_2$,0_2-0_2**,** etc absorption in the solar spectrum. It is just that for the wavelength region that the report focuses on (I presume the visible), $N0_2$ is the primary absorber.

Pg 67 second line: should be B_{ab}, not B_{ap}

Pg 68 last line: Does "localized events" mean "near the source?"

Pg 74: 5.4.4: should be "calculating" not "reconstructing"
Pg 74: above eq 5-2: should be M^2/ g not g/M^2 . Also 0.6 M^2/g is pretty crude, should mention about the uncertainty in this number

Pg 75 first full paragraph: Malm reference is Huffman (1 996b). Also, there are two Huff-man papers (back to back; 1996a and 1996b). The title in the reference list is wrong.

Pg 76 second to last line: As discussed above, this much absorption disagrees with about 50 years of data. There is significant doubt about the accuracy of these measurements (Heintzenberg et al 1997).

Chapter 6

Pg 80 last line: again "calculation" not "reconstruction"

Pg 81 Table 6- 1: MOVE THIS TO AN APPENDIX

Pg 89 first full paragraph: again "calculated" not "reconstructed"

Pg 89 first bullet: Reference should be Huffman (1996)a

Cha
pter
7

Pg 100 first line: What is. ATAD?

Cha
pter
8

Pg 112 first paragraph: This is nice dodge, but total nonsense. The last sentence would do a lawyer proud.

Pg 113 second paragraph: Delete the paragraph – it's obvious

Pg 113 fourth paragraph: Text has four dispersion an two receptor - but 7 models are listed.

Pg 116 second full paragraph: Delete - it's unnecessary and verbose.

Pg 117-127: Seems like some of this could be trimmed i

Pg 131 first full paragraph, first sentence: This sentence needs to be reworked. Why not "We could not agree on a procedure to evaluate the methods."

Pg 132 8.5: This is tacked on and is totally out of place here. How about putting the image processing stuff in its own chapter or appendix.

Chapter 9

Generally - I was confused about the difference between Chapter 9 and Chapter 10 (conclusions). It seemed that Chapter 9 contains conclusions and Chapter 10 is superfluous.

Sections 9.1-9.2: These sections are unnecessary and should be deleted. If needed for political reasons, reference previous work.

Pg 140 second paragraph from the bottom, first sentence: This sentence needs to be made clearer. I figured it out after reading it five times.

Pg 142, first bullet: I have no idea what they are trying to say here.

Pg 144, first full paragraph: Last half of the paragraph repeats what was

said on page 131. If necessary here, at least change the wording.

Pg 147, Table 9-2: I have no idea what that is supposed to show.

Pg 157, 9.8: Again, this is tacked on here. Give the perception folks Their own chapter or put it all in the Appendix.

Chapter 10

Pg 163, Figure 10-1,2: These figures are interesting and important but should be somewhere else. Perhaps in a reworked Chapter 9/10 they will read better.

Pg 164, last paragraph: reference is made to photographs.

Pa 165, 10.2: What a weak ending - move this to an appendix.

References

Clean up several references: DOE, Huffman (I 996ab), Seigneur twice, Sloane.

Again all reports that are not available in a technical library should attached.

Review of Project MOHAVE Report
Phil Hopke, Clarkson University

This report does a good job of honestly reporting the results of the field study and the likely bounds on the contribution of the Mohave Power Plant (MPP) to sulfate concentrations in the Grand Canyon National Park (GCNP). It particularly portrays the inability of any of the source-oriented models to provide any useful quantitative representation of the effects of the MPP on the GCNP. However, it does appear that more could be done with the tracer and other data to determine the impacts of the plant and determine the uncertainty bounds on those estimates.

It is clear that considerable effort and resources went into the modeling of the transport of the emissions from the MPP to the downwind receptor sites. The inability of these models to agree on when and how much the effects of the plant are on the downwind air quality is presented, but then there is a considerable subsequent effort made to make something out of these results in spite of their clearly limited value. It appears that this effort has taken away time from what might have been alternative approaches which I will outline below that might have been more useful.

In section 8 (page 112), it is claimed that a receptor modeling approach "has no predictive capability for other times and locations." If this is indeed so, then the efforts to renormalize the dispersion models using the PFT concentrations leads to an identical situation. How do we know that the modified models have any validity for other times and locations? If receptor models can only be applied within the domain of the measured data, then the same restrictions must hold for the dispersion models when they have been forced to fit for the tracer data. In fact this problem is acknowledged later in the document. However, it would seem appropriate in both cases to say that for similar physical situations for the modeled system, (e.g., similar emissions, similar meteorology), the results of the modeling effort whether receptor or modified dispersion should be appropriate estimates of the contributions of the sources on the likely receptor site concentrations.

I agree with those in the project group that recommend that the cumulative frequency distributions NOT be shown. Given their substantial disagreements on which sampling intervals are affected and which are not as well as the extent of the impacts, it is really not appropriate to use the amount of space to present these results as they are given. It may be useful to put these into an appendix with the appropriate caveats, but even with the disclaimers presented in the text, it really is inappropriate to make much of them. They also should be redrawn with the probability axis as a probability axis so one can immediately look for their distributional properties. If they are really "frequency" distributions, then they should be plotted against a probability axis.

On page 154, if "For periods without tracer data, it cannot be reliably known whether MPP emissions are reaching Grand Canyon National Park," then why show figures 9-20 to 9-21 and the related discussions since the results are not reliable? It really seems that there is too much of an effort to salvage the unreclaimable failure of the models to predict the dispersion of the MPP emissions in this region. It obviously disappointing to everyone that the complex terrain and resulting flow patterns make the models unreliable, but that is really the only valid conclusion that comes out of the all of the modeling efforts and no amount of after-the-fact massaging is going to change that fundamental outcome. Better to be done with it up front and see what more can be done to make maximum use of these data.

Suggestions for Additional Analyses:

First, one can test the utility of the tracer to predict sulfate by separating the data into two data sets. One set is used to develop the predictive model for sulfate as a function of measured tracer. The validity of the model can be tested by applying the model to the test data set. One can use resampling techniques to assess confidence intervals on results. This problem can really be conceived to be a multivariate calibration problem and an appropriate model built to determine the receptor site sulfate as a function of the concentrations of the various PFTs and other variables such as elemental concentrations. Since there is a constant ratio of the MPP tracer to emitted SO2, the model can then be adapted to predict the amount of receptor site sulfate mass per unit mass of emitted SO2.

Second, there could be more use made of the trajectory data. There are several methods that have been employed in the past to examine the relationship between source locations and receptor concentrations. A particularly promising method, Residence Time Weighted Concentrations, was reported by Stohl (A. Stohl, Atmospheric Environ. 1996, 30, 579-587.) which would provide a quantitative estimate of the contributions of upwind sources to the observed concentrations of an ensemble of samples. The trajectories could also be used in Residence Time Analysis, Areas of Influence Analysis, Quantitative Bias Trajectory Analysis, and Potential Source Contribution Function, as well as Residence Time Weighted Concentrations.

Third, it is stated on page 137 that the MPP is along the same line as transport from Los Angeles. This provides another opportunity to use the data collected over a long time interval to estimate the incremental influence of the MPP. Since the early 1980s, the South Coast Air Quality Management District has aggressively reduced the amount of SO2 within the Southern California Air Basin. Thus, if trajectories are used to select those samples in the Grand Canyon area that should have been impacted by both LA and MPP, one could examine the trends in this series. Since the MPP contribution should be relatively constant while the LA contribution has been substantially declining, it should be possible to get an estimate of the relatively constant MPP signal in the declining one from LA. Thus, rather than complaining about the geographical collinearity on page 137, use it to make an estimate.

Minor Points

On page 128 where the TAGIT results are first presented, there should be a discussion of where the negative results come from. It can be found in Chapter 9, but it should be here where the negative results are first presented.

Top of Page 139, “Winter Haze” is repeated.

Second Review

I have examined the revised version of the Project MOHAVE final report. I must say that I am quite disappointed that you chose to retain the frequency distributions. I believe that they are highly inappropriate and quite misleading in portraying the results of the modeling efforts. Given the significant disparity between paired values derived by different models, I think the careful reader will find the problem, but the more casual observer may not fully recognize the problems that have been seen in modeling the flow field under these difficult terrain conditions. I recognize that all I can do is provide my advice, but I really feel you are doing yourself a disservice by portraying the results in this inappropriate manner. I hope that you will willing to reconsider this point.

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Review of MOHAVE Final Report

Prepared by Jonathan D. W. Kahl
6 November 1998

General comments:

Overall, I find the report document to be clear, concise, and well-prepared. Clearly Project MOHAVE was a large-scale effort involving a significant amount of complexity, both in the scientific issues addressed and in the technical approaches used to investigate these issues. In general, I believe the assumptions are valid, the analytical methods are sound, and the conclusions are defensible, especially since there is an appropriate level of attention and detail devoted to discussions of the uncertainties in the various methods, and in the levels of confidence that can be placed on the resulting conclusions.

The report is quite readable, and appears to be internally consistent. I found a few errors in figure numbering and in references to cited literature, but these are exceptions to an otherwise tight document. The key technical findings are consistent with those that appear in the executive summary. Upon inspecting the images with artificially-adjusted light extinction levels, contained in the cdrom, I find that a change in light extinction of +/- 10% is just barely perceptible, not more. However, since there are a number of large S02 sources contributing to visibility degradation in the Grand Canyon, I would not label such a change as insignificant.

The report could be improved by assessing the representativeness of the MPP contributions to Grand Canyon light extinction during 1992, as opposed to other years. As described in my comments below, it is not clear that 1992 is truly a representative year.

One aspect of the report that I find particularly interesting and robust is that in the highly complex terrain of the southwestern US, tracer measurements are critical for accurately determining source-receptor relationships and evaluating future emissions scenarios.

Specific comments:

Section 3.2, Tracer Release Network

> The EI Centro and Tehachapi Pass tracer release sites were selected in order to "bracket" the Los Angeles and San Diego urban source areas. In hindsight, it may have been more appropriate to place these tracer release sites downwind of LA and San Diego, rather than slightly north and south. This downwind placement may have provided more direct estimates of southern California urban contributions.

Section 5.4.4, Reconstructing total extinction from components:

> On p. 76 a reference is made to "White, 1993" - this reference doesn't appear in the bibliography.

Section 7. 1. 1. Meteorology:

> It is not clear why the 15-year period (1977-1982 and 1984-1992) was chosen to evaluate the climatological representativeness of the 1992 target year. As stated in the text. 30 years is the typical time span used to construct climatological averages and to identify anomalous years.

The report states "it was determined that 1992 was a 'typical' year and that both winter and summer were 'typical' seasons in a mix of 'atypical' years and seasons." This statement, along with the selection of 15 years' as the base period from which to evaluate the representativeness of the 1992 year, is not justified in the text. What variables were used? What methodologies? The report states that "These results were accepted and peer reviewed by the meteorology subcommittee of the GCVTC", but no reference is given. It is well-known that interannual variability in basic meteorological parameters such as frequency of wind direction, clouds and precipitation can affect source-receptor relationships as much as the source parameters themselves (i.e. emissions strength). In a study involving only one year, it is important to clearly demonstrate the representativeness of the year under study, and this has not been done to my satisfaction. In addition, no method (or discussion) has been presented to aggregate or extrapolate the 1992 results to other years, which are described in the text to be "atypical" (atypical seems to be the norm, and typical the exception!).

Section 7.1.2, Light Extinction:

According to the frequency distributions of light extinction at the Grand Canyon shown in figures 7-1 - 7-4. 1992 does not appear to be a typical year. At the south rim and in-canyon, 1991 was the haziest year of the 8 winters shown (Figures 7-2 and 7-4). In the summer, the in-canyon light extinction was nearly the lowest of the years shown, and was in fact the lowest at the 80d' percentile and above (Figure 7-3). As emphasized later in section 10. 1 (and illustrated by equation 10-1), the light extinction is the bottom line of the investigation, yet little attention is given to the apparent non-representativeness of 1992 in terms of this parameter.

Section 7.2, How does the meteorology which affects sulfate concentration, their contributors, and haze levels differ ihroughout the year and from year to year?

An analysis of the interannual variation in transport (i.e. multi-year trajectory analyses) is promised in the section 7.2 heading, but is not delivered in the report. I believe this analysis does belong in the report, because the representativeness of 1992 in terms of transport (the most important meteorological variable) is a crucial point, especially since 1992 doesn't appear to be typical in terms of bext the most important physical variable.

Section 7.3.2, Particulate Sulfur Trends:

The report states: "During this same period (mid 1980's), the Nacozari smelter entered service in Mexico. This smelter location is far enough south and east of the Tonto and Chiricahua Class I areas that its emissions don't affect the air quality at those locations frequently. Rather, the generally westerly flow carries its emissions toward Big Bead National Park...". I don't completely accept this argument. The tracer patterns (Figures 7-9 and 7-10) show generally S & SW flow during summer. The sulfur from the Mexican plants may well be affecting the study area samples, even if transport is not direct (i.e. recirculated). This could be verified by forward trajectory analysis from the Mexican sources, or better yet by releasing tracer there if that were possible. It seems quite possible that the "unqualified increase" in Mexican emissions may have counteracted the decrease in SW S02 emissions shown in Figure 7-11.

Second Review

4 March 1999

I have looked at the Feb 22 project MOHAVE draft report.

Overall I find the report to be a clear, well-prepared document. In my 6 November 1998 review of the earlier draft I raised a few specific concerns which I feel have not been adequately addressed:

- The representativeness of 1992 has still not been demonstrated to my satisfaction. The draft explains (page 7-1) that "a chi-squared analysis was performed and then each season was comparatively ranked", and cites "Farber (1995)", an unpublished report. This is little more than asking the reader to exercise blind faith that appropriate analyses were preformed correctly.
- The lack of a discussion of the interannual variation in transport (i.e., multi-year trajectory analyses) compounds this problem.

Other than that, I feel that my comments have been addressed satisfactorily.

PROJECT MOHAVE REVIEW COMMENTS

By

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Review submitted to:

Ms. Mary Ann Allan
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November 13, 1998

Project Mohave Report - Comments from R. Ian Sykes, Titan/ARAP

This report describes an extensive study of a very complex problem, and my overall impression is that the scientific approach has been very thorough, and that the conclusions are sound. Full use has been made of all the available data, and the report properly discusses the difficulties and limitations associated with the various approaches. I think the report does a good job of supporting the conclusions, and is cautious where necessary. Since my own area of expertise covers transport and diffusion, my technical comments are focussed on Sections 8 and 9. As a non-expert, I found the other sections to be coherent, and also reasonably clear and convincing. The discussion of the difficulties with both measurements and modeling are clearly explained, and complicating factors such as multiple sources are addressed. I particularly appreciated the use of multiple approaches, using the wide range of available data, to provide independent estimates. I think this greatly strengthens the conclusions.

There are, not surprisingly, a number of areas where I think the discussion could be clarified. I don't believe any of the issues to be discussed below would materially affect the conclusions, but they seemed to raise questions in my mind. There are also a few typographical errors listed below.

Technical Points

Section 8.3. The overall modeling approach and philosophy is difficult to understand. Apparently, the initial models (DRI/CSU, HAZEPUFF, and VISHWA) show poor performance, as indicated by the comparison statistics in Table 8-1. On the basis of this poor comparison, a set of final modeling approaches is described in Section 8.3, which are claimed to make use of the PFT tracer information (p116). The only modification to a model that I see is for HAZEPUFF, where the stability assumptions were modified to improve the bias, and it is implied that wind input was changed from the initial results. However, since the wind data is not described for the initial study, the reader cannot judge its importance. In addition, only one set of model comparison statistics is given for the modified HAZEPUFF, and that is for Meadview 24hr data. Since the initial statistics are only given for Meadview 12hr and Hopi Point 12hr, there is no basis for assessing the improvement.

The other dispersion models are apparently dropped in favor of CALPUFF and HOTMAC/RAPRAD, but there is no discussion of the rationale for this. The stated aim of the "final" models is the improvement of performance through use of the PFT data, but I don't see much justification for this. I presume the use of PFT data for CALPUFF is indirect, similar to the HAZEPUFF modifications, where various options were tested and the best results chosen. However, there are no performance statistics for CALPUFF. There are only limited results for HOTMAC/RAPTAD, and they don't appear to be much better than the "initial" models.

Since model performance for space-time correlated measurements is generally poor, due to plume trajectory errors, it would be enlightening to show cumulative frequency plots for the tracer measurements versus the various models (especially CALPUFF which figures heavily in Section 9). This would provide some perspective on the corresponding cumulative frequency plots in Section 9, which are essentially the bottom line result. As the report points out, we expect to be able to predict a conserved tracer better than the reactive sulfate, which also depends on getting the chemical reaction rates correctly modeled in addition to transport and diffusion.

Sections 7.2 and 8.3.11 both discuss trajectories without any specific information on the definition. Are they two-dimensional trajectories? If so, what vertical level is used? If they are three-dimensional, then are they isentropic or do they use a direct measure of the vertical velocity? In any event, the vertical diffusion of the species implies that some kind of average velocity will be effective in transporting the material.

Also, I had great difficulty with the windfield assessment in 8.3.11 and the paper in Appendix C-7. This is not a critical part of the study, fortunately, because I don't see how any quantitative measure of accuracy can be obtained. The trajectory measure depends completely on the distribution of tracer measurements, since the "tracer potential" is proportional to the measured

tracer concentration. Any trajectory that passes close to a high concentration measurement will receive a large positive score, while missing lower concentration points (further away from the source, presumably) is not penalized strongly, but this doesn't necessarily imply that it is a good trajectory overall.

p58. Why isn't 1/10/92 the highest measurement at GRCW. Why plot it if it is excluded?

p103, last para. It is slightly misleading to say that the average dispersion results "illustrate the typical flow patterns". They certainly reflect the typical flow patterns, but the typical day (if there is such a thing) may be very different from the average.

p108. In reference to Figure 7-12, I don't see any noticeable reduction in the inter-site variability in the later years. Is there some quantitative measure?

p130. Figure 8-6 shows a comparison between the HAZEPUFF estimate for the sulfate time series at Hopi Point and the "tracer max" estimate. The comparison doesn't convey much information, since the "tracer max" signal stops before HAZEPUFF shows any non-zero values. Is this the best comparison that can be plotted? It certainly shows a positive result in that HAZEPUFF predicts zero when no tracer was present, but there isn't much information there.

Editorial Notes
p (ii) - LOME isn't in the legend for Fig A

p29. Table legend (3-8) at the bottom of the page should be on next page.

p52. Table legend (4-4) at the bottom of the page should be on next page.

p54. Table legend (4-5) at the bottom of the page should be on next page.

p107. References to both Figure and Table 7.3-1 are incorrect. The figure should be 7-11, and the table should be 7-1.

p122. The first sentence seems to be incomplete.

p139. "Winter Haze" is repeated on the first line.

Appendix C. It would be very helpful to have the 13 papers in Appendix C listed by name and page number at the beginning of the Appendix. It is extremely difficult to locate any particular paper.

GENERAL COMMENTS:

The MOHAVE project was conducted by several groups of well qualified and experienced scientists with expertise in the areas of study design, particle sampling and analysis, air pollution meteorology, and source attribution. The report reflects the high caliber of work produced by the participating groups. It is well written and clearly organized, and the different sections are well integrated. In addition, there are a large number of figures and tables that present the data nicely. My only comment on the writing is that the report should have been more concise, particularly the last two sections.

With regard to the technical aspect of the report, the sampling and analysis methods used for the MOHAVE project were excellent, especially considering that the study was designed almost nine years ago. Most of the data collected were of high quality and the detection limits were very low, which was necessary due to the low concentration levels encountered at the monitoring sites. The accuracy and precision of the trace element and ionic species measurements were quite good. The investigators have examined systematically the results from the replicate measurements and the method intercomparison studies. These results suggest that the selected sampling and analysis techniques were satisfactory for most of the parameters; however, this was not true for the sulfur dioxide and organic carbon measurements. Sampling artifacts for organic carbon may have been responsible for overestimation or underestimation of its concentrations. In addition, both the DRI and UC Davis analytical approaches were based on assumptions that are difficult to validate. To date, the sampling and analysis of elemental and organic carbon is still problematic and there is a need to develop accurate methods. Therefore, it was not possible for the investigators to select better methods for organic carbon sampling and analysis.

The sampling and analysis procedures for the tracer measurements were well thought out. The precision and accuracy of the measurements are very good. Moreover, controlling the tracer release so that it is proportional to the fuel consumption of the power plant was a good idea; however, the concept of using the tracer technique to determine the impact of the Mohave Power Plant (MPP) is not sound for the following reasons:

(1) The use of a gaseous tracer to investigate the formation and transport of particulate sulfates is problematic, considering that we will still need to know the deposition velocities for sulfur dioxide and sulfate and the oxidation rate of sulfur dioxide. To determine the maximum source impact, the investigators assumed that no deposition takes place for the two sulfur compounds and that all sulfur dioxide is converted to sulfate. Although this estimation sets an upper boundary that may have a physical meaning, it is unrealistic. Furthermore, other source attribution models took into account the deposition and oxidation processes and used different values for deposition velocities and oxidation rates; however, no sensitivity analysis was conducted to illustrate the effect of the selected values on the resulting model calculations. One would expect that even small differences in oxidation rates or deposition velocities can make a big difference.

(2) The tracer approach used by the investigators indirectly implies that the MPP emissions mix with clean air (sulfur free air). If no sulfur compounds were released by the plant and the tracer was observed at the receptor site, the sulfate concentration should have been zero. One could argue about the accuracy of the tracer technique for this specific experiment, considering the fact that the sulfate concentrations at the Meadview site are very low in general (few hundreds of ng/m3), and that emissions from the MPP mix with ambient air (which, at a minimum, contains few hundreds of ng/m3 of sulfate). Of course this concern is invalid when concentrations at the receptor are much higher and the expected source impacts are more pronounced (for example, a more proximate source).

(3) One of the tracers was used to investigate the impact of a large area source (California emissions). It is not possible to use a point tracer source to track emissions from a large area. Pollutants move vertically

and horizontally; thus, releasing a tracer between the source and the receptor does not guarantee that the source emissions will mix with the tracer prior to their arrival to the receptor. Therefore, this approach presents many limitations.

(4) Tracer concentrations were not much higher than background concentrations, which were variable. This may have a significant effect on the accuracy of the emission impacts on the receptor. Thus, the regression analysis results (regression of visibility data or sulfate concentrations on the tracer concentrations) may cause an underestimation of the source impacts on the receptor.

Because of the reasons mentioned above, I believe the tracer approach is inadequate for providing accurate determinations of the impacts of the MPP emissions. I agree with the investigators that the tracer was useful in tracking the primary emissions of the MPP and in demonstrating the inaccuracies of the dispersion models. Certainly this added value to the study; however, one should be clear that it is not possible to use tracers for investigating the impact of a secondary pollutant on a specific site(s). This was probably the most important lesson of the study and I was surprised that it was not stressed in the conclusion section, entitled "What technical lessons were learned as a result of the Project MOHAVE?"

I think the real lesson learned from this study is that, in spite of the hard work, the involvement of talented scientists, and the use of the best available tools, the study does not provide accurate information to air quality managers. Although a monumental effort was made by the investigators to explain their diverging findings by being creative and thoughtful, one cannot ignore the fact that the results of this very expensive study are qualitative, ranging between zeros and unrealistic maximums. This may be an interesting scientific exercise but, from an air quality management perspective, these findings do not provide a solid basis for decision-making. I am certain that for years to come, both sides will argue about their interpretations of these results. Investigators may disagree about the upper and lower limits of these model calculations, as illustrated by the manuscripts prepared by the participant groups. The bottom line is that an objective scientist cannot come to definitive conclusions based on these findings. Although the data of this study provide ample evidence to support this, the closing remarks of this report have failed to make this point.

The limitations of the study are due to the lack of available sampling, chemical analyses, and computational tools that can be used to model complex physico-chemical processes adequately, such as the formation and transport of secondary pollutants, including the sulfate species. This is also recognized by the National Research Council's Report on Ambient Particles, which calls for more research in order to enhance our understanding of source receptor relationships.

Technically, I believe that the study is sound in terms of its design, sampling and analysis methods, and data quality. Also, I believe that the investigators are competent and made a good effort to meet the objectives of the study. My biggest concern is with the data analysis. I believe the data have been overinterpreted. Similarly, the tracer approach has some advantages but it was sold (and presented in the report) as the panacea to answer any question. Furthermore, there is a lack of connection between the findings and the conclusions. There are many sections of the report which discuss the shortcomings of the study and the limitations of the different models, and this should be kept in mind when decisions are made based upon these results. It is my opinion that the scientific information provided by this study is insufficient for making decisions regarding the impact of the MPP. That does not mean that the findings should not be published. In fact, I believe that they raise a number of methodological issues which can be of great value to the scientific community and will be very helpful in designing future source attribution studies.

SPECIFIC COMMENTS:

Page i (Executive summary), first line: Instead of "technical opinions" use "data interpretation."

Page ii, six lines from the top: The physical constraints (100% conversion and no deposition) are very far from reality. Although the obtained results have a physical meaning, their value is limited.

Page iii (Overview), twelve lines from the bottom: "……..but also had higher low concentrations and lower high concentrations." Use statistical terms (e.g., percentiles) to present these results.

Page vi, twenty-one lines from the top: Did the investigators take into account particle hygroscopicity (e.g., particle acidity and in general particle composition) to determine the effect of relative humidity on aerosol size?

Page ix, thirteen lines from the bottom: "…depositional loss." Specify if this loss is for sulfur dioxide, sulfate, or both.

Page 17, Table 3-3: The measurement of sulfur dioxide from the IMPROVE sampler is not very accurate due to its interaction with alkaline coarse particles on the Teflon filter.

Page 20, sixteen lines from the top: ". . . amount of sulfur from the MPP." It is better to say sulfur dioxide or total sulfur. This is just a detail.

Page 21, nine lines from the top: The investigators released tracers at the Tehchapi Pass and near El Centro in the southern Imperial valley to trace emissions from the Los Angeles Basin. Use of two small point sources to trace emissions from large area is not the most sound approach (as already mentioned in the general comments section). It is possible that the receptor site can be impacted by sources located in the Los Angeles basin whose emissions did not necessarily mix with the released tracers at the two specific locations. Furthermore, it is certain that sources located in the Los Angeles basin contribute to the background concentrations in this area. Overall determination of the impact of the Los Angeles sources is not trivial and the approach followed here is too simplistic.

Page 28, eight lines from the bottom: It is important to know the variability of the background tracer concentrations. This will make it possible to determine the lowest tracer concentration which can be attributed to a specific source. Eliminating the "elevated values" is not a rigorous scientific approach. Furthermore, based on the results presented in Table 3-8 one can conclude that the observed tracer concentrations were not that much higher than the background ones.

As mentioned in the general comments section, an important issue which is not addressed in this report is the sulfate background concentration at the MPP. Even in the case where the MPP emissions did not contain any sulfur dioxide or sulfate, one would expect sulfur species to be present at the receptor site when the plant tracer is detected. This simply happens because MPP emissions would be diluted with air masses that contain these species. Since the concentrations at the receptor were low, one would expect that the effect of the sulfate background should not be neglected.

Page 34, sixteen lines from the top: I am unsure whether the atmospheric variability has anything to do with LQLs.

Page 42, discussion on sulfur dioxide losses: The authors should mention that sulfur dioxide losses on the inlet of the IMPROVE sampler depend upon the relative humidity and the surface properties of the inlet.

Page 43: The purpose of using the Quartz after filter is questionable. To date, it is not clear whether the second quartz filter collects gas phase organics or volatilized particulate organic carbon lost from the first filter. Carbonaceous particles were collected on a series of two quartz filters. Presumably, the first collects all of the particle phase carbon (elemental and organic) and a very small fraction of gas phase organic carbon. Likewise, the second quartz filter is supposed to collect only the same small fraction of gas phase organic carbon as collected by the first filter. Therefore, the concentration or particle phase organic carbon is calculated by subtracting the amount of organic carbon on the second from that of the first stage; however, this assumption does not take into account particle phase organic carbon that can volatilize from the first filter and get collected by the second quartz filter. When this volatilization occurs, it results in underestimation of particle phase organic carbon.

A large fraction of organic carbon consists of semi-volatile species which are partitioned between the between particle and gas phases. This equilibrium depends on temperature, vapor pressures of individual species, and the amounts and types of adsorbing particle surfaces. The amount of the particulate phase of these species volatized from the (first) filter depends upon sampling conditions. Therefore, the approach of using two quartz filters (as done by the IMPROVE sampler) is questionable. Eatough, et al. (1989) concluded that desorption of organic gases from particles on the first quartz filter was the dominant sampling artifact (negative artifact), while Turpin, et al. (1994) suggested that organic gases can be collected by the quartz filter (positive artifact). Turpin, et al., found that adsorbed organic gases represent up to 50% of the organic carbon measured on quartz filters in southern California. This study suggested that: (1) organic gas absorption (positive bias) was much larger than organic particle volatilization (negative bias); (2) as sample durations increase, the fraction of the adsorption bias decreases because the filter becomes saturated; and (3) the magnitude of the bias depends on the composition of the organic gases and particles present in the air sample. For these reasons, we currently do not know to what extent the use of the second quartz filter improves the accuracy of the organic carbon measurements. Overall this is a problem that has nothing to do with the study, but it really has to do with the availability of an accurate sampling technique.

Page 44, seven lines from the top: The investigators claim that for the light extinction budgets based on the multiple regression method, the loss of volatile material may not change the extinction attribute of carbon. This would be correct if the percent loss remains constant; however, this is untrue since the loss of the semi-volatile carbon depends on temperature and particle concentration, as well as other physico -chemical parameters, as mentioned above. As a result of the loss variability, the regression slope for carbon will probably be underestimated and the intercept will be overestimated.

Page 45, four lines from the top: The reviewer agrees with the investigators that the value of 10 m^2/g seems more realistic than the values of 5 and 20 m^2/g.

Page 46, Figures 4-1: The agreement between the PIXE and IC is impressive. Dr. Cahil and his group have done a remarkable job of improving the accuracy of the PIXE method.

Page 46, eighteen lines from the bottom: As mentioned above, the purpose of the afterfilter is questionable and this is supported by these results.

Page 46, thirteen lines from the bottom: I personally think that a fraction of fine particulate matter consists of crystalline water which is present even after the filter sample is dried out. If this is true, one would expect that OMH is overestimated; however, both OMH and OMC methods have many flaws, so it may not be worthwhile to explain why they do not agree.

Page 47, Figure 4-2, left: It is possible that the slope is influenced by the two high concentration points. Also, it would be helpful to include the number of observations, n, in all figures presenting the results from the regression analysis.

Page 47, nine lines from the bottom: There is no basis for the assumption that 5% of sulfate is present in the form of sulfuric acid. This percentage varies with season and usually is higher.

Page 50, Figure 4-6: The agreement for the iron and zinc is remarkable. Again, the results from the PIXE measurements are impressive. It would be helpful to include a table with all the trace elements comparisons.

Page 51, fifteen lines from the bottom: This Taylor test may be a useful statistical tool, but from the analytical chemistry point of view it is not a rigorous approach.

Page 59, general comment: As mentioned above, the idea of using a gaseous tracer to investigate the emission, transport, and impact of a particulate species is unappealing, especially if this species (sulfate) is a secondary pollutant.

Page 59, Figure 4-7: This is not the best way to present the collocated measurements. A second (side by side) x/y plot may be more revealing of the method precision. The same comment applies to the next Figure 4-8.

Page 78, four lines from the bottom: The IMPROVE sampler does not use an ammonia denuder to protect the acidic particles (collected on the Teflon filter) from acid neutralization. In addition, no precautions were taken to protect the samples during transport and storage. Thus, the ammonium/sulfate ratio reported by the investigators is higher than it should be and probably is not accurate. To date, denuder/filter pack techniques have been used to measure particle acidity and other ionic species (Koutrakis, et al., 1992). The samplers consist of three components: (1) a PM2.5 inertial impactor to remove coarse particles (which are generally alkaline and would consequently neutralize the fine particle strong acidity collected on the sample filter); (2) a diffusion denuder to remove gaseous ammonia from the air sample; and (3) a Teflon filter to collect fine particles.

Page 78, eleven lines from the top: Andrews, et al., 1998 provides a comprehensive discussion on this topic.

Page 89, four lines from the top: State which of the two extinction coefficients (measured or reconstructed) is larger. Also, considering all the measurement and modeling uncertainties, the agreement between the measured and predicted extinction coefficients is quite good.

Page 95, ten lines from the top: Figures 7-1 through 7-4 are essential in order to evaluate the representativeness of the air monitoring periods. The investigators did a very good job in contrasting the data from the previous years.

Page 109, seven lines from the bottom: These results are not surprising. Indeed, many believe that sulfate levels depend to some extent on the availability of atmospheric oxidants, the concentration of which has steadily increased. In addition, no one knows the impact of the Mexican emissions.

Page 112, eight lines from the top: Disagreement between the model results and the tracer concentrations does not necessarily indicate that the models are wrong. As I mentioned above, the tracer approach has its own shortcomings.

Page 121, section 8.3.1 Tracer Max: The validity of the upper bound estimates is questionable. This is also underlined by the investigators in the last three lines of the 8.3.1 section; however, this is not reflected in the attached manuscript entitled, "Estimating the contribution of tile Mohave Coal-Fired Power plant Emissions to Atmospheric Sulfur at Grand Canyon National Park," by Rogers and Malm. For example, see the last four lines of the abstract of this manuscript.

Page 122, section 8.3.3, Tracer Regression: This is a crude method which lacks scientific rigor.
Same page, next section: The TAGIT approach seems to be more realistic than the previous two, although it too has its shortcomings.

Page 123, section 8.3.5 Modified CMB: The profiles of the different regional emissions tend to present many similarities. In statistical terms: the source profiles are not orthogonal. Because of these collinearities one would expect that resolving the profiles of regional emissions may be a difficult task.

Page 128, twenty two lines from the bottom: Looking at these results one can realize how qualitative the findings of the MOHAVE project are.

Page 131, sixteen lines from the bottom: "The viewer of these plots must bear in mind. . . ." I believe that comparing frequency plots is misleading for the exact reason mentioned by the authors. When you have methods that aren't in agreement and their estimates are bounded between zero and some upper limit, one would expect that the frequency distributions would be similar, although the outcomes of these methods are not correlated. Thus, frequency plot comparisons can be misleading. I hope air quality managers will not base their decisions on
these plots.

Page 135, section 9: The discussion in this section fails to address the questions raised by the authors. I found the answers long and confusing. This section should be more concise.

Page 139, first line: Fix typographical error.

Page 141, nine lines from the top: The fact that no association was found between MPP tracer concentrations and light scattering does not necessarily mean that MPP emissions do not contribute to the light extinction coefficient.

Page 142, Figure 9-3: An x/y plot or some statistical analysis that compares measured and observed values may be useful. Figure 9-3 is not self-explanatory. It seems that in some cases measured and observed values differ, but it is hard to say.

Page 143, four lines from the top: This is untrue! Do you mean gaseous emissions? The term "primary fine particulate matter is confusing and incorrect.

Page 145, Figure, 9-5: From a purely scientific point of view I believe this figure has little value, primarily for two reasons: (1) there is too much disagreement among the different methods; and (2) comparing frequency plots is misleading. Also, from the air quality management point of view this figure helps very little in establishing sound control strategies.

Page 165, section 10.2: This section does not reflect the quality of the report. The conclusions presented here are unsupported by the findings of the study. They fell short in capturing the primary findings of an important study, and in offering helpful suggestions for future ones.

REFERENCES:

Andrews, E., Saxena, P., Musarra, S., Hildemann, L.M., Koutrakis, P., McMurry, P.H., Olmez, I. and White W.H. "Concentration and Composition of Atmospheric Aerosols from the 1995 SEAVS Experiment and a Review of the Closure Between Chemical and Gravirnetric Measurements." Submitted to *Journal of the Air & Waste Management Association.*

Eatough, D.L., Sedar, B., Lewis, E.A., Hansen, E.A., Farber, R.J. (1989). 'Determination of Semi- volatile Organic Compounds in Particles in the Grand Canyon Area." *Aerosol Science and Technology, 10,* 438-456.

Koutrakis, P., Wolfson, J.M., Thompson, K.M., Spengler, J.D., Keeler, J.G., and Slater, J.L.(1992) "Determination of Aerosol Strong Acidity Losses Due to Interaction of Collected Particles: Results from Laboratory and Field Studies." *Atmospheric Environment.* 26A, 987-995.

Turpin, B.J., Huntzicker, JJ, Hering, S.V. (1994). "Investigation of Organic Aerosol Sampling artifacts in the Los Angeles basin." *Atmospheric Environment,* 28, 19, 3061-307 1.,

Tony Wexler

Comments on Draft Project MOHAVE Final Report

Introduction

My knowledge of the Mohave Power Plant, visibility issues at the Grand Canyon National Park, and Project MOHAVE were quite limited coming into this review. My procedure in this review was to first read the executive summary to develop a focus. Then I read almost every page of the report looking for the assumptions employed and evidence supporting the executive summary conclusions. Finally I re-read the executive summary to review the conclusions in light of the knowledge that I gained from the report.

As requested, I have organized my comments in two categories. The first category addresses topics oriented towards the overall report quality, project science, and conclusions. The second category contains either typographical errors and other minor comments, or suggestions regarding clarity of presentation. Although some of my comments could lead to alternate approaches or reanalysis of the data, I realize that this is beyond the scope of the changes that can be made to the report.

Overall Comments

O.1. The report is clearly written and thoroughly presented. Assumptions are differentiated from observations and conclusions.

O.2. Project MOHAVE seems to be predicated on the premise that a link between secondary particulate sulfate from MPP and visibility impairment at the GCNP will establish the degree that MPP is responsible for the visibility reductions. The report places very little emphasis on other compounds emitted by MPP that may cause visibility impairment such as primary PM and secondary organic vapors. Bullet 4 on page v of the Executive Summary states that dispersion modeling demonstrated that primary particulate concentrations are not sufficient to cause noticeable visibility impairment, yet the report does not appear to establish this. Organics also receive insufficient attention. Figures 6-6 and 6-7 show that the organics contribution to visibility impairment is relatively low at the site but error bars are not placed on these data. The error bars on the organics may be quite large considering the paucity of accurate methods for determining their aerosol concentrations.

O.3. One of the conclusions of the report concerns the fraction of visibility impairment attributable to MPP emissions. This fraction will change due to changes in emissions from MPP or the other significant sources in the region. The relative importance of MPP emissions will increase if the regional sources reduce over time whereas its importance will diminish if the regional sources increase over time. One would expect emissions to decrease on the US side of the border for the foreseeable future, but those in Mexico are more uncertain. This perspective is the key for weighing the future impact of MPP and the report would benefit by including projected emissions from MPP and the regional sources that affect its background.

O.4. A large fraction of the references employed in the report have not been peer reviewed or published in readily available journals. This is unfortunate considering the comments in this regard received by the EPA on the last PM criteria document. If material presented in abstracts or at conferences has also been presented in a peer-reviewed venue, the references should be changed to reflect this where possible.

O.5. At many locations in the report, the point is made that emissions from southern California pass over the MPP before reaching the GCNP. This poses two problems for the visibility assessment. First, it obscures the contribution of MPP given the substantial background. Are there tracers indicative of MPP (such as a trace metal) that could be used, or could have

been used, to distinguish it from the background? Second, the southern California plume may significantly alter the oxidizing capacity of the air that is diluting the MPP plume. This could be very important to emission controls for MPP. The relationship between SO2 emissions and sulfate production is a nonlinear one so it is not clear what that anticipated relationship is between SO2 emission reductions and sulfate concentrations at GCNP, let alone visibility improvements. Due to difficulties associated with complex terrain, cloud processing events, and measurements, the models were not in very good agreement with the measurements. Yet some statistical features of the model predictions exhibited significant agreement when averaged appropriately. It seems appropriate to run them with reduced emissions in conjunction with reduced, enhanced and current background to assess what if any effect reductions might have.

O.6. Much is made of the lack of agreement between the transport modeling and tracer efforts, and "blame" is placed firmly on the models. Are there any possible contributions to this discrepancy from the measurements?

Specific Comments

S.1. Page 4. SO2 emissions are reported in different units, tons/day and tons/year. Use consistent units.

S.2. Page 10, Table 2-1. The component PM2.5 masses do not agree with the total PM2.5 mass. The disagreements should be addressed.

S.3. Page 18, Section 3.1.3. The first paragraph of this section implies that nitric acid vapor was not measured. Is this true?

S.4. Page 38, section 4.2.1.1. Significant effort is placed on quantifying the variation in PM2.5 cutpoint at different locations but the slope of the cutpoint is never specified.

S.5. Page 42, Sulfur Dioxide. Considering the biased and uncertain SO2 data, some effort should be placed here on the overall implications for the conclusions, or alternatively refer to later sections where this may compromise the conclusions, if any.

S.6. Page 42, Sulfate: Does the reference to Eatough et al., 1997 pertain to 1997a or 1997b?

S.7. Page 43, line 2 ñ remove "the"

S.8. Page 67 section 5.3. Remove "path" on line 3 (there are two occurrences)

S.9. Page 68 paragraph 1. Why are the extinction coefficients higher in the canyon? If the composition of the air is the same, the RH should be lower at the bottom of the canyon due to higher temperatures, which should lead to lower extinctions there. These data appear to be evidence that either the instruments are significantly biased or that the concentration of PM is significantly higher in the canyon. Flows channeled down the canyon from local sources is the presumed cause, but this may not be supported by the meteorological patterns. Is the channel flow so efficient at bringing locally emitted pollutants into the canyon that it always causes this difference? If so, is this fairly direct evidence that MPP emissions are reaching the canyon in the summer and significantly degrading visibility ñ how could regional sources lead to such a difference? If not, are the instruments biased?

S.10. Page 74, paragraph 3. The units of the scattering efficiency (0.6 g/m2) are inverted.

S.11. Page 107. The text refers to Figure 7.3-1 and Table 7.3-1, which should be Figure 7-11 and Table 7-1, respectively.

S.12. Page 122. The beginning of the first paragraph is missing.

S.13. Page 124. Section 8.3.6. Is the Malm reference to 1989 a or b?

S.14. Page 131, line 2 ñ insert the word “with”

S.15. Page 137, paragraph 3. Is the first Vasconcelos reference 1996 a or b? The Vasconcelos, 1998 reference is not in the reference section. Also in paragraph 2 on page 140.

S.16. Page 139, first line. Remove “Winter Haze”